Author responses to Reviewer #2 ('Comment on hess-2021-512)

1. The authors of this manuscript analyzed the temporal variability in the isotopic composition of rain water and snow samples, and quantified the contribution of glacial melt water to stream runoff, by means of stable water isotopes, in three study catchments in the Swiss Alps.

The topic of this manuscript is potentially interesting for the readers of Hydrology and Earth System Sciences. In general, I think that more studies investigating the contribution of snowmelt and glacier melt to stream runoff in high elevation catchments are needed to improve our understanding of hydrological processes in such complex areas. Overall, the paper is well structured and well written, but I have several (major) concerns about the methodological approach.

Firstly, the authors have not considered the contribution of groundwater to runoff both in the accumulation and the ablation period. Groundwater is expected to be the dominant end-member during the accumulation period, but a large contribution of groundwater to runoff may be possible from the glacier-free areas of the catchments during the ablation period.

Secondly, more details are needed in the section 2.4 about the hydrograph separation. The authors should explain the choice of the end members, provide the assumptions at the base of the hydrograph separation technique (please see Klaus and McDonnell, 2013), and describe how uncertainty was estimated (it is mentioned only at lines 420-423).

We thank Reviewer #2 for his/her important comments. As mentioned in the general response to the reviewer's comments, we acknowledge that our approach of neglecting groundwater as significant interim storage for glacial melt, rainwater, and snowmelt was somewhat simplified and that our dataset does not allow making strong quantitative statements regarding the glacial meltwater contribution to mountainous streams for all hydrological set-ups. Therefore, we plan to slightly shift the scope of the manuscript and we intend to focus more on the opportunities, challenges, and limitations of using stable water isotopes to quantify the contribution of glacial meltwater to mountainous streams. This will also include a more detailed uncertainty analysis of the hydrograph separation in the monitored stream discharges and the corresponding isotope signatures if we are allowed to revise the manuscript.

2. Thirdly, the authors should consider more and discuss the temporal and spatial variability in the isotopic composition of the end members. Previous studies conducted in Alpine catchments (e.g., Schmieder et al., 2016; Schmieder et al., 2018; Zuecco et al., 2019) have already shown that a high spatial and temporal variability in the tracer composition of the end members can greatly affect the results of the hydrograph separation and/or hamper its application. In this study, the authors used only three samples of glacier ice (and from only one of the glaciers) to characterize the glacier-melt end member. This sample size is too small for making any consideration on hydrograph separation.

In our opinion the strength of our manuscript is the isotopic analysis of the snow samples collected during a consecutive period of 13 months (February 2019 -March 2020), whereby during the winter months the samples could only be collected via the usage of helicopters. Moreover, a high number samples was also collected in the streams at the outlet of the three catchments over a 10 months period. We think that the collected snow and stream samples in our study provide additional information to previous studies (e.g. Müller et al., 2021;

Schmieder et al., 2016; Zuecco et al., 2019) such that they provide new insights into the processes causing the temporal variation of the stable isotope signature of alpine snow packs (Fig. 4). For this reason, we plan to provide an extended discussion regarding this important topic in the revised version of the manuscript if we are allowed to revise the manuscript.

Regarding the glacier-melt endmember, the reviewer is right that the number of samples is low. Nevertheless, since it has been previously shown that the isotopic variability of glacial melt is rather low, at least when compared to that of snow (Müller et al., 2021; Schmieder et al., 2018; Zuecco et al., 2019), Hence, we think that we captured the isotopic variability of the glacial meltwater with our samples and that they are representative for the glacial meltwater in the mountainous streams.

3. Finally, the authors have not described which approach was used to assess the end of the snowmelt period in the three catchments (using snow cover data collected at only one station at 2063 m a.s.l. is not sufficient).

We agree with the reviewer that it is challenging to define the end of the snowmelt period. We chose the end of July because this is roughly one month after the snow cover disappeared at the snow cover measurement station (Fig. 3). Thus, for catchments with subsurface residence times of less than a month, most of the snowmelt that was stored in the subsurface has left the catchment. The observation that the quantified meltwater generation for August to September yields reasonable values for the Steinwasser catchment (see the general response to the reviewer’s comments) suggests that the chosen approach including the choice of the end of the snowmelt period is reasonable. Our quantification approach provides reasonable estimates either because the groundwater contribution during this period was low or because the corresponding subsurface residence time is short (in the order of less than month). This discussion will be added to the revised version of the manuscript if we are allowed to revise the manuscript.

4. The introduction is mainly focused on the role of hydropower in Alpine catchments, whereas there is too little attention towards the application of tracers in high-elevation catchments to quantify the contribution of glacier-melt water to stream runoff."

We agree that the introduction was biased towards the role of hydropower. Since we plan to slightly change the scope of the manuscript, the introduction will be adapted according to the revised scope of the manuscript if we are allowed to revise the manuscript.

5. Lines 47-48: This concept repeats the text at lines 32-35

Thanks for indicating that. We plan to update the introduction such that it will be in line with the anticipated shifted scope of the manuscript if we are allowed to revise the manuscript.

6. Line 54: I would not describe the tracer-based methods as low cost compared to other methods, such as hydrological modeling.

We agree that tracer-based methods are labor-intensive, particularly because of the remote and hardly accessible terrain. Thus, we will remove the statement that the tracer-based methods are low-cost methods in the revised manuscript if we are allowed to revise the manuscript.

7. In the legend of Figure 1, I suggest indicating the glacierized area.

In Figure 1, the glaciated areas are shown in lighter colors than the rest of the three catchments. This will be described in the updated figure caption in the revised manuscript if we are allowed to revise the manuscript.

8. Line 123: 19 snow samples is not a high sample size.

We see the point of the reviewer and we will remove this characterization of the snow sample number. Instead, we will emphasize that the snow samples were collected during more than an entire year (February 2019 – March 2020), which is one of the main novelty of our sampling campaign.

9. Line 131: I suggest indicating the number of ice samples that were collected.

We agree with this suggestion and we will indicate the number of the collected ice samples in the revised manuscript if we are allowed to revise the manuscript.

10. Lines 132-133: Three samples collected at the glacier fronts cannot be representative of the whole ablation zone. Additional samples are needed to support the main findings of this manuscript.

See the response to comment 2.

11. Lines 274-276: These two sentences are not supported by rain samples collected during the accumulation period.

In Figure 2, we did not report any rainwater isotopic data for the accumulation period. Therefore, we do not fully understand what the reviewer means here.

12. Lines 363-371 and Figure 6: I suggest comparing discharge values after normalization by catchment areas.

We agree with the reviewer and we will apply the suggested change when preparing the revised version of the manuscript if we are allowed to revise the manuscript.

13. Lines 410-411: The author should provide evidence about the presence/absence of snowmelt in all three catchments during the ablation period.

In our monitoring period, we recorded two minor snowfall events in early September and mid October 2019 (<10 cm). The fresh snow has quickly molten on the next day. At the end of the ablation period on November 10, 2019 (Fig. 3), no snow was present in the three catchments based on webcam monitoring of the area at the Susten Pass at an altitude of 2224 masl. We will add this information to the revised manuscript if we are allowed to revise the manuscript.

14. Lines 420-423: These sentences belong to section 2.4.

We agree that these sentences should be part of the method section. We plan to move them to the method section when preparing the revised manuscript if we are allowed to revise the manuscript.

15. Figure 8: This figure could be interesting if more catchments were considered; is it possible to gather data from other Alpine catchments? If not, I suggest deleting the figure

As described in in the general response to the reviewer's comments, we plan to slightly change the scope of the manuscript and we will no longer provide fully quantitative estimates of glacial meltwater contribution for the Giglibach and the Wendenwasser catchment discharge. Therefore, Figure 8 will be removed when preparing the revised version of the manuscript if we are allowed to revise the manuscript.

16. Line 172: It is unclear what the authors mean with "binary mixing approach". I suggest using another term, such as "two-component hydrograph separation"

We used a binary mixing model with two end-members and this is the reason why the term "binary mixing approach" was used. However, we have no issues with changing the term as suggested. This will be done when preparing the revised manuscript if we are allowed to revise the manuscript.

17. Line 223: Please indicate the water source for "heavy isotopes".

The source relates to rainwater, which condensates and precipitates at lower altitude. This information will be added in brackets when preparing the revised manuscript if we are allowed to revise the manuscript.

18. Lines 225-226: Please mention the water source considered in the sentence

The water source refers to rainwater and this information will be added to the sentence when preparing the revised manuscript if we are allowed to revise the manuscript.

19. Figure 4: Please indicate in the caption what the error bars represent.

The error bars represent the analytical uncertainty of δ^2H and δ^{18}O as described in the method section. For δ^{18}O it is 0.10 ‰, for δ^2H it is 1.5‰. This information will be added when preparing the revised manuscript if we are allowed to revise the manuscript.

20. Figure 5: Please indicate in the caption what the error bars represent.

The error bars represent the analytical uncertainty of δ^2H and δ^{18}O as described in the method section. For δ^{18}O it is 0.10 ‰, for δ^2H it is 1.5‰. This information will be added when preparing the revised manuscript if we are allowed to revise the manuscript.

References

Müller T., Schaefli B. and Lane S. N. (2021) Assessing the effect of the geomorphological complexity of glacier forefields on the multi‐temporal water dynamics will provide better future models. *EGU General Assembly 2021, EGU21‐7182, 10.5194/egusphere‐egu21‐7182, 2021.*

Schmieder J., Garvelmann J., Marke T. and Strasser U. (2018) Spatio-temporal tracer variability in the glacier melt end-member — How does it affect hydrograph separation results? *Hydrological Processes* **32**, 1828-1843.

Schmieder J., Hanzer F., Marke T., Garvelmann J., Warscher M., Kunstmann H. and Strasser U. (2016) The importance of snowmelt spatiotemporal variability for isotope-based hydrograph separation in a high-elevation catchment. *Hydrol. Earth Syst. Sci.* **20**, 5015-5033.

Zuecco G., Carturan L., De Blasi F., Seppi R., Zanoner T., Penna D., Borga M., Carton A. and Dalla Fontana G. (2019) Understanding hydrological processes in glacierized catchments: Evidence and implications of highly variable isotopic and electrical conductivity data. *Hydrological Processes* **33**, 816-832.